with clinical experience of the full range of medical problems of elderly people.¹

There are no reasons to suppose that clinical indications for anticoagulation are different in elderly and younger patients. The differences that exist are in management and contraindications. The differences in dosage are, as pointed out by Dr Lowe, caused by pharmacodynamic changes with age. Control is not more difficult as long as the doctor is aware of the changes. Patients should not be denied useful treatment because their doctors have experience of failure because of their own or their juniors' lack of knowledge of changes in dosage with age.

It is because of multiple illnesses that contraindications to anticoagulation are more common in elderly people, not because of age alone. Cardiac failure and depression are common among elderly people, and these conditions make control particularly difficult.² Dementia occurs in a fifth of elderly people, and these are the patients who "are not good at taking drugs reliably or accurately." The other four fifths are as good at this or sometimes better than those who are younger.

It is difficult to generalise about the use of anticoagulants in elderly people. Because of the range of indications and contraindications each patient must be considered individually. This is often a difficult and time consuming process which unfortunately many doctors avoid by taking the nihilistic option.

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Screening of vision in school

We would like to add support to the findings of Drs S L Stewart-Brown and M Haslum (29 October, p 1111) with some results from a questionnaire sent to school nurses in Islington, an inner London health district (table).

Number of nurses carrying out each test in children of different ages

Test	Children's age at testing (years)			
	5	7	9	11
<i>Distant acuity</i>				
Linear chart	3	3	6	9
Stycar	9	2		
Keystone		7	6	1
<i>Near acuity</i>				
Snellen				1
Stycar	11	5	5	8
Keystone		5	6	1
<i>Colour vision</i>				
Ishihara	10	3	4	9
Guy's	1			1
Keystone		7	6	

Within this district there was a lack of uniformity when testing for visual defects, along with inappropriate use of the tests. Distant visual acuity should always be tested with a linear chart and is possible for all schoolchildren.¹ Measurement of visual acuity with the single optotype booklet is not always accurate.² The Snellen near vision chart may be used by most children and the Stycar chart need be introduced only when the child cannot name letters and needs to match them. Colour vision testing is being carried out far too frequently.

Some nurses were testing at an incorrect distance of 3 m, not all tested eyes separately, and a card

held by the child (allowing peeping) was often used for occlusion.

We are now providing nurses with the correct test material, clear instructions for use, and strict referral criteria and reviewing testing techniques regularly.

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A fair hearing

Undoubtedly, an audiology service for the elderly organised within the community health service can not only be the better but also the easier and cheaper way to help elderly deaf people (19 November, p 1289).

In Haringey, north London, a comprehensive audiology service for children existed for many years. When the ear, nose, and throat department at the Prince of Wales's Hospital closed down the audiology centre for children was asked to take over the hearing aid service for adults. At first our chief audiology technician tried to deal with the long waiting list for hearing aids, which was an almost impossible task. We were able to start additional audiology sessions at a local health centre. Soon afterwards we found that close cooperation with general practitioners speeded up the process of examination, diagnosis, and the issuing of hearing aids to their patients: some patients were sent directly from the surgery to the audiology clinic along the corridor. We realised that there was an overwhelmingly urgent need to do something for the many elderly patients waiting months for examination and even a year for their hearing aids and decided to extend our audiology service for children to adults and elderly people. We were warned that "you cannot just start a clinic without permission from the higher authorities."

Because the need was so great we could not wait, and within a fortnight we sent for the first patients (five to six per session). They were examined and tested, and those who needed an aid had an impression taken and within two to three weeks were issued with the most suitable instrument (it is nonsense to say that NHS aids are no good. There is a wide range for selection and some are better than many bought privately.) We encouraged the wife, husband, or relations of the patient to attend. A booklet on how to talk to the elderly deaf was given to them. A few weeks later the patient was seen again, difficulties were sorted out, and further advice given.

The chief technician started a domiciliary service for those who could not attend the clinic. All this was so successful that everyone concerned—social services, etc—was astonished that such a service could be introduced speedily without much fuss and expense.

This service still exists (apart from the domiciliary service, which had to stop recently when the chief audiology technician retired and no replacement could be obtained). Those in need of ear, nose, and throat treatment are referred to the local ear, nose, and throat department, and a good relationship has been established with the consultant.

Help to the elderly who are hard of hearing can be provided with comparatively inexpensive resources, especially when an audiology service for children already exists within the community health service. It is advantageous if the services of an audiological physician can be obtained, but there are now numerous senior medical officers

in the community health service who are well trained in audiology (they even have their own organisation—the British Association of Community Doctors in Audiology).

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The monstrous regiment

I was interested to read Dr Ann Bowman's recent remarks about the reluctance of female general practitioners to work full time (17 September, p 740), and I would like to add a few points.

Firstly, a full time woman general practitioner must be resident in the practice area to fulfil on call commitments. If because of her husband's job she is not it means renting a flat for on call work. A part time job—that is, five full days a week—might be all that were possible.

Secondly, it is not surprising that there were only four female applicants for the full time vacancy recently advertised in Dr Bowman's practice. The advertisement said, "Shrinking violets need not apply."

I was one of those four female doctors, and I applied because I am available and already resident within the practice area. I was not even granted an interview.

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The debate over mass mammography in Britain

The short article by Dr Petr Skrabanek (15 October, p 971) attempting to argue the case against a national breast screening programme is in his inimitable style, mixing errors and distortion of the published evidence. The list is extensive; the following are examples.

(1) "The overall mortality in the Swedish trial in the screened group is higher than in the control group." At the end of 1987 the reverse was true with a 1% lower total mortality in the group allocated to screening.

(2) "The yearly benefit was one death fewer in each 15 000 women screened." At the end of 1987, there were about 70 fewer deaths due to breast cancer in the study group than expected based on the control group. In the five years 1982-7, in which the deficit of breast cancer deaths was observed, the yearly benefit was one breast cancer death fewer for each 5500 women allocated to be screened; in the age group 50-64, on which the programme in this country is concentrating, the yearly benefit was almost one death fewer for each 4000 women in the study group.

(3) A positive predictive value of only 5-10% might be expected from the results of the Canadian trial. He does not mention the estimates for the positive predictive value derived from any of the European programmes, such as the Swedish trial or Nijmegen programme, both of which he quotes in other contexts. For women over 50 these estimates range from 40% to 50%, a figure at which to aim in the United Kingdom programme. It would result in some 5000 mammograms a year showing false positive results, leading to a biopsy, rather than the 65 000 put forward by Dr Skrabanek.

(4) "The authors of the Swedish study reported 30-40% overdiagnosis in the screened group"; to the end of 1985, 16.7 cancers per 1000 women had been diagnosed in the study group compared with 13.5 in the control group, an excess of 24% (these figures include ductal carcinoma in situ). This excess, however, has nothing to do with overdiagnosis. Dr Skrabanek seems not to have

understood that the purpose of screening is to diagnose cancers earlier. Thus in the first year of a programme it is hoped to diagnose not only cancers that would surface spontaneously in that year but also many that would otherwise have surfaced in the second, third, and fourth years of the programme. The excess of cancers in the screened group results from the earlier diagnosis. Other evidence shows that overdiagnosis has not occurred in either the Health Insurance Plan study or the Swedish studies.

Dr Skrabanek has a valid point that incomplete accounts of the Swedish study have been published internationally. This reflects, however, editorial reluctance to publish extensive statistical data rather than an unwillingness to publish by the investigators. Your readers, however, may find strange Dr Skrabanek's complaint that some of the results of the Swedish trial appeared in the Swedish literature.

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AUTHOR'S REPLY.—Firstly, in the last published analysis, which Dr Day coauthored, the eight year cumulative death rates per 10 000 women in the screened and control groups were, respectively, 935.7 and 917.3 for all deaths except breast cancer and 21.7 and 30.5 for deaths from breast cancer. This shows a net excess of deaths in the screened group.¹ The data to which Dr Day refers in his letter on the position at the end of 1987 are not referenced and presumably unpublished.

Secondly, the table shows the yearly benefit of mammography in the two counties trial¹ and the randomised Swedish trial from Malmö (15 October, p 943).

Yearly benefit of mammography in two counties and Malmö trials

		Two counties	Malmö
No of woman years	{Study	545 471	186 297
	{Control	389 157	187 016
Breast cancer deaths	{Study	124	63
	{Control	119	66
Absolute risk reduction of dying from breast cancer		0.008%	0.001%
No of women screened for each death from breast cancer prevented by screening		12 755	67 568

Thirdly, the positive predictive value of mammography in the two counties trial is exaggerated as it was not based on the ratio of the number of cancers detected to the number of women initially positive. On that basis the positive predictive value works out at about 12% (A B Miller, personal communication). What is more relevant, however, is the positive predictive value achievable in Britain. Using a specificity of 92%, as obtained recently in the Royal Marsden Hospital, and assuming a prevalence of three breast cancers per 1000 women screened, the implementation of the Forrest report would lead to over 100 000 false positive mammograms a year.²

Fourthly, Dr Day takes exception to a statement that runs in an article which he coauthored as follows: "There is still [seven years after randomisation] a 30% excess of invasive cancers diagnosed in the study group compared with the control group."³ As this excluded ductal carcinoma in situ the total excess was in fact 15.0 (study) minus 10.9 (control) per 1000 women—that is, 38%. In the Malmö study this excess was 30% and in the Edinburgh-Guildford trials 51%.

Dr Day's belief that the excess of cancers is carried over from the first year is not supported by any evidence. As to his belief that it has "nothing to do with overdiagnosis," his Swedish collaborators

clearly showed that in one arm of the trial (Kopparberg) there were about twice as many breast operations than in the control county—not only in the first year but every year.⁴ Dr Day does not explain why in the Health Insurance Plan trial, which did not show any significant excess of breast cancers in the screened group, the reported benefit was the largest of all randomised trials. Surely the theory of screening would expect that the higher the "excess" in screened women the more "early" cancers and the greater the benefit? Yet the published evidence shows that the opposite is true. Is there some fundamental flaw in the theory itself?

Fifthly, although Dr Day concedes me "a valid point," he does so for wrong reasons. What was objectionable in the reporting of the two counties trial was not the absence of "extensive statistical data" but the suppression of essential information. The reporting of the Malmö trial in the *BMJ* was exemplary in providing all the necessary information without a welter of confusing statistics.

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Mastectomy or conservation: the patient's choice

Dr P J D K Dawes and others (5 November, p 1167) stated that random allocation to conservation or mastectomy might have distressed the patients and increased "the currently low psychological morbidity of the unit." They claimed that these reservations were later justified when an attempt by "the Medical Research Council's Clinical Research Centre to do just such a trial failed for these reasons."¹ They are actually referring to the Cancer Research Campaign's breast conservation study, which ceased recruiting patients because of low accrual in 1985. There was absolutely no evidence supporting the imputation by Dr Dawes and others that random allocation to treatment with informed consent in itself caused the psychological morbidity found among patients in that trial. Rates of anxiety and depression were similar to those found in other studies without randomisation. The low accrual was due to the insistence of the working party on fully informed consent, about which many potential participants felt uncomfortable.²

The authors' assertion that most patients selected mastectomy for domestic and employment reasons was based on interviews with only 28 women more than two years after they had chosen their treatment. We have no information as to how these 28 women (18.3% of the total study sample) were chosen to be interviewed; how many of the 28 had had a mastectomy; how many were treated with breast conservation; and how many, if any, had experienced recurrence of their disease. It is important that we are told how many of the sample refused to be interviewed before deciding whether the views of the 18.3% interviewed represent those of all the other patients in the study. It would also be useful to know if the interviewer used a semistructured interview schedule or recorded any of these discussions for independent raters to determine whether patients were satisfied with treatment.

We find the fact that none of the women were unable to make a decision about treatment choice somewhat surprising in the light of our own experience. The unpublished study we are currently conducting suggests that a significant minority of women prefer the decision to be taken by the doctor and therefore true autonomy means permitting the patients the option of delegating the choice to the surgeon.

Finally, may we comment on the treatments between which the patients were choosing? We note that the tape-slide presentations that were used to help the patients in making their decision have been in use since 1979. During that time there have been several studies suggesting that adjuvant treatment, particularly tamoxifen for postmenopausal and chemotherapy for premenopausal women, conveys a significant survival advantage.^{3,4} Both these treatments were denied the Newcastle patients. We would also be interested to hear of the scientific basis for the assertions that women with in situ lesions should not be offered conservation treatment.

The issue of patient choice is an extremely important one to study rigorously as giving patients responsibility for the choice of treatment also gives them responsibility for outcome. Merely reporting that most of the 28 out of 153 women who were given a choice did not regret that choice is not sufficient. The work of Leon Festinger on cognitive dissonance would predict such a result.⁵ Moreover, the perception of choices and consequent decisions are dependent on how the facts or issues in question are framed.⁶ Subtle biases may creep into the most diligent researcher's work, especially if the data are based entirely on subjective assessments of interviews with patients.

We concur with the authors, however, that the anxiety and depression associated with breast cancer and its treatment might be alleviated in the short term by offering choice whenever possible but that this can be examined only with a prospective study with a satisfactory follow up period and well validated, standardised tests. We also agree that the assumption that when given the choice all women wish to retain their breasts is clearly misplaced.

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AUTHORS' REPLY.—Firstly, we must apologise for transposing Medical Research Council and Cancer Research Campaign and also for seemingly misinterpreting the significance of the poor recruitment to the trial and the subsequent results of the study of Fallowfield *et al.*

In Newcastle the word "choice" has been used since the inception of this concept of involving the patient in the selection of her treatment, and the difference between "choice" and "option" is almost certainly semantic in this instance.

We hoped that we had made it clear in the paper that the group of 28 interviewed patients was a pilot sample only and was in no way a formal study.